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ABSTRACT

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Abstract

In true experiments in which sample material can be randomly assigned to treatment conditions, most researchers presume that the condition of equal sample sizes is statistically desirable. When one or more a priori contrasts can be identified which represent a few overriding experimental concerns, however, allocating sample material unequally will lead to more powerful tests of certain hypotheses, when the assumptions underlying the analysis of variance are known to be met. The purpose of this study was to develop systematic procedures for assigning a fixed amount of sample material to treatment conditions using either of two optimality criteria: (a) the maximization of the average precision of a given set of contrasts, or (b) the equalization of precision across a set of such contrasts. On the basis of relevant literature, the squared standard error of the estimate was adopted as the appropriate measure to minimize or equalize. Algorithms were developed mathematically to achieve each of these goals under specified conditions in single factor, fixed effects, analysis of variance applications.

Allocating Sample Material to Increase the Precision of a Priori Contrasts

Introduction

In true experiments in which experimental units can be randomly assigned to treatment conditions, most researchers assume that the condition of equal sample sizes is statistically desirable. When the researcher can identify one or more a priori contrasts which represent a few overriding experimental concerns, however, allocating sample material unequally will lead to more powerful tests of certain hypotheses.

According to Cohen (1969, p. 8), "Experimental design is an area of inquiry wholly devoted to the removal of irrelevant sources of variability for the increase of precision and therefore for the increase of the statistical power of tests of null hypotheses." In other words, "best designs are those which in some sense minimize the errors associated with making the set of estimates which is the goal of the experiment" (Ash & Hedayat, 1978, p. 1296). It follows, then, that attempts to develop systematic methods for increasing the precision of statistical tests--thereby decreasing experimentally irrelevant variability and increasing power--are congruent with the goal to which experimental design dedicates itself.

Perspectives

Although largely ignored in much educational and psychological research, convincing arguments have long been advanced exhorting the investigator to consider experimental design issues before collecting any data. Foci of concern have included sample size determination, precision of estimates and/or power, effect size, and Type I error rate. In addition, it has been suggested that the constraint of limited experimental material which faces many researchers who conduct confirmatory research is not only acceptable but even desirable, since it tends to result in parsimonious investigations of well-thought-out hypotheses and fewer rejections of trivially false null hypotheses.

One technique which has been used in educational and psychological research to achieve more powerful tests of hypotheses is that of planned comparisons. Its superiority in power--compared to post hoc procedures--is well established. Essential to their use is the ability of the researcher to state, a priori, a few hypotheses which dictate the interpretation of the whole experiment. By doing so, the investigator may diminish the probability of making a Type II error (compared to post hoc procedures), while still maintaining a stringent control over Type I errors. The identification of situations in which planned comparisons are more appropriate than post hoc techniques has consumed

much textbook and journal space. In general, experts agree that planned comparisons should be used in those studies for which the importance of not committing a Type II error is greater than the loss of information inherent in not being able to "data snoop."

Another central issue in the wise use of planned comparisons is the identification of a few research questions. Due to the nature of the procedure, attention must be paid to the setting of the alpha level when more than one a priori hypothesis is tested. Awareness of the relationship among familywise error rate, experimentwise error rate, and error rate per comparison is important to an understanding of the impact of testing multiple contrasts on the probability of committing a Type I error. Generally, however, an accepted practice is to modify the alpha level for each comparison using a Bonferroni adjustment. This is true even when the contrasts are not mathematically orthogonal--as when sample sizes are unequal--if the degrees of freedom for the error term are large, i.e., greater than 40 (Games, 1971b, p. 533; Hsu, 1980, p. 32). Under the condition of nonorthogonality, the Bonferroni adjustment is conservative, so any error will not prejudice the conclusions toward rejection of the null hypothesis (Perlmutter & Myers, 1973, p. 182).

Although well-chosen planned comparisons (i.e., which are congruent with relationships found in the population) are more powerful than post hoc techniques (as long as the number of subjects is reasonably large), the advantage is diminished by each additional contrast tested, in accordance with the procedure cited above. Hence, much attention is devoted to the need for the researcher to limit his/her hypotheses:

> Each esoteric issue posed to the data costs a [degree of freedom] which is lost from the error estimate, thus enfeebling the statistical power of the analysis. This, ultimately, is the reason that it is desirable in research that is to lead to conclusions to state hypotheses which are relatively few in number. (Cohen, 1968, p. 442)

After the researcher has framed a few well-chosen questions and translated them into a priori contrasts, the steps which should be taken next, in order to select the "best" design, depend on his/her (implicit or explicit) reactions to issues raised in the area of optimal design.

Optimal Design

The area of optimal design is multifaceted and convoluted. An overview of the literature which appeared in an issue of Communications in Statistics devoted entirely to

optimal design theory contains no fewer than 312 references, along with a caveat that several major areas in optimal design were excluded (Ash & Hedayat, 1978). A primary focus in this body of literature is the selection of the parameter which is to be optimized. The number of different criteria seems to be exceeded only by the number of authors who support each. Although the techniques are based on complex mathematical theory, the applications are iterative and sequential, fitting models to empirical data ex post facto (Box & Wilson, 1951; Hill & Hunter, 1966). Since most optimization research has developed outside the social sciences, many of the issues addressed by theorists in optimal design are of little concern to investigators of social and psychological phenomena.

A primary area of mutual concern seems to be the desirability of selecting experimental designs which maximize precision, i.e., minimize some measure of error variance (Ash & Hedayat, 1978, p. 1296; Bock, 1975, p. 307; Box & Wilson, 1951, p. 40; Feldt, 1958, p. 340; Finney, 1960, p. 11; Plackett & Burman, 1946, p. 307; Wald, 1943, p. 134):

> By ruling out some of the error variance from the observations one is <u>decreasing</u> the standard error of the mean, and thus <u>increasing</u> the power of the test against whatever [alternate] hypothesis is true. Experiments in which the variability attributable to experimental or sampling error is

> small are said to be PRECISE; the result of such precision is that the experimenter is quite likely to be able to detect when something of interest is happening. (Hays, 1981, p. 254)

An appraisal of the power of a statistical test requires that the researcher establish a definite effect size of interest or that he/she adopt a reasonable relative effect size, as suggested by Cohen (1962, p. 153). The former approach requires some knowledge of the magnitude of the within-group population variance. Though the feasibility of obtaining a reliable estimate of the error variance has been questioned, many authorities feel that such estimates are available. Possible sources for this information include previous research and pilot studies. Miller & Knapp suggest that the underlying problem may <u>not</u> be the unavailability of accurate estimates but a reluctance to use others' estimates. This, they feel, is a result of the customary emphasis on the <u>uniqueness</u> of an author's contribution, and it "tends to weaken, rather than strengthen, the continuity of science and the relevance of new discoveries" (pp. 22-23).

Assuming that a reasonably good estimate of the within-group population variance is secured and that power

analyses can be conducted, the avenues to achieving increased precision are varied. Among the options available is the allocation of sample material across the treatment conditions. This is generally advisable only when planned comparisons constitute the primary focus of the experiment (Overall & Dalal, 1965, p. 345).

Equal Versus Unequal Allocation

Proponents of the practice of allocating experimental material equally across treatment conditions regardless of the research questions, typically offer varied arguments for doing so: for ease of computation, to maintain the orthogonality of the statistical tests, to maximize the power of the omnibus F, or to take advantage of the robustness of many statistical tests to violations of the assumptions underlying the analysis of variance. On the other hand, some authorities feel that circumstances exist in which the investigator should consider allocating sample material unevenly. If there is differential interest in the research hypotheses, unequal sample sizes may be advisable (Appelbaum & Cramer, 1974, p. 336; Cox, 1958, p. 190; Kiefer, 1959, p. 279). Because the greatest risk in unequal allocation lies in the increased importance of the assumptions underlying the analysis of variance (due to the robustness issue), it is imperative that the effects

of such violations be examined.

The extensive literature on the effects of violations of the assumptions underlying the analysis of variance leaves little doubt that, by far, the most inviolable postulate (with the exception of the independence of observations) is that of homoscedasticity. Whereas departures from additivity and normality have negligible--or at least predictable--effects on outcomes, when within-group population variances are heterogeneous, the effect on Type I error rate and power depends on the distribution of ns relative to the magnitude of the within-group variances. When comparisons involving more than two groups are involved, the effect becomes increasingly difficult to predict. The condition of heterogeneous variance coupled with unequal ns, then, poses a formidable problem for the investigator. Also, as Glass, Peckham, and Sanders point out, power analyses cannot be conducted under the condition of variance heterogeneity, since the computation of the noncentrality parameter is impossible (1972, p. 273).

Constraints of Study

Several general assumptions have been made in connection with this study which are consistent with recommendations made in the literature:

1. The research focus is presumed to be confirmatory

in nature, with a few planned comparisons identified which represent the primary impetus of the investigation. Also, the investigation can be considered a true experiment, with sample material randomly assigned to treatment conditions.

2. The assumptions of homoscedasticity, normality, independence, and additivity are tenable. Evidence as to the deleterious consequences of the failure to meet these assumptions in the condition of unequal sample sizes is overwhelming (Glass, Peckham, & Sanders, 1972).
3. There are constraints on the number of experimental units which can be utilized in the investigation.

Several additional conditions have been specified, either in an effort to limit the focus of the study or for the sake of mathematical convenience:

1. The cost of applying treatments is either constant across treatment conditions or it is not a factor.
2. The effect size of interest is constant across contrasts and is classified as "medium." This is consistent with recommendations made by Cohen and others and represents a reasonable treatment effect for educational and psychological research.
3. The sum of the absolute values of the coefficients

> for each contrast is two. Although the choice of two is somewhat arbitrary, it is desirable that the sum of the absolute values of the coefficients be equal for all contrasts, so that the interpretation of effect size be the same regardless of the number of groups involved in the comparison (Hsu, 1980, p. 35).

A Comparison of Three Approaches to Allocating Sample Material

The alternative algorithms for assigning experimental material to treatment conditions which are developed in Appendices A and B are not presumed to be panaceas for the researcher. Certainly, the two imply that the objectives of the researcher are very different: (a) a desire to maximize the chances of rejecting at least one null hypothesis (which will be referred to as "Option II"), versus (b) a desire that each research question be tested with the same degree of precision, allowing (a) to be secondary (this will be called "Option III"). Since it is assumed that the researcher has formed several a priori hypotheses and that his/her objectives coincide with those of one of the alternative methods, the decision becomes a matter of weighing the relative advantages of equal and unequal allocation: Does the increase in precision offered by Option II--compared to allocating material equally (which

will be referred to as "Option I")--compensate for the potential difficulties associated with unequal sample sizes? Is the appeal of testing each hypothesis with the same degree of precision (i.e., Option III), great enough to warrant the probable loss in power?

Power Analysis for Planned Comparisons

Glass, Peckham, and Sanders, in a significant article dealing with the consequences of the failure to meet the assumptions underlying analyses of variance and covariance, argue strongly against the practice of using techniques of computer simulation to examine issues regarding the power of, and extent of agreement between, actual and theoretical distributions of test stastistics (1972, pp. 239, 282). In the present context, since the relationships among alpha level, sample size, effect size, and power are well defined, a comparison of the three allocation schemes can be carried out mathematically, making computer simulation "potentially misleading . . . [compared to] actual inferential applications" (Glass et al., 1972, p. 282). The nature of contrasts is such that they may be viewed as comparisons between two quantities--the weighted means represented by the positive coefficients and those represented by the negative coefficients. This is also apparent in the F-test which is applied to planned comparisons, i.e., there is one numerator degree of freedom. Hsu (1980) has used

this fact to generate probability distributions for nonpairwise contrasts under the condition of equal ns. Using his notion of the effect size of a contrast as "the difference between two means expressed in σ units" (p. 36), an extension can be made to the case of unequal ns:

$$\phi = (\mu_p - \mu_n) \Big/ \left[\sum^{k} (c_{ij}^2 / n_j) \right]^{\frac{1}{2}},$$

where ϕ is the noncentrality parameter in the t distribution, $(\mu_p - \mu_n)$ is the standardized difference between the positively weighted and negatively weighted means, and the denominator is the standard error of the contrast. The calculation of ϕ, along with Cohen's recommendation that, in the absence of other imperatives, an alpha rate of 0.05 and a "medium" effect size be adopted as reasonable standards, makes possible a mathematical determination of power. In this determination of power, however, it is necessary to ascertain probabilities associated with varying noncentral t distributions. This was accomplished in the present study using a Supplemental Procedure of the Statistical Analysis System (Hardison, Quade, & Langston, 1981), in conjunction with a computer program which performs the preliminary calculations and processes the results. In order to maintain the experimentwise error rate at conventional levels (i.e., 0.05), the alpha level employed for each comparison was calculated using a Bonferroni adjustment.

Results and Discussion

Table 3 through 14 in Appendix C summarize the results of comparisons of precision of the three different allocation schemes for selected levels of k and n in conjunction with varying coefficient matrices. The standard error of the estimate and power for each contrast are reported, along with means and standard deviations for both, across each set of contrasts.

One does not have to scrutinize the tables in Appendix C too closely to realize that the algorithm which presumes to maximize the average precision of a set of contrasts (Option II), results in a consistent shortfall in power--compared to equal allocations--when the contrasts are of the Helmert type (see Tables 5, 6, 11, and 12). This seemingly incongruous outcome is due to a rather troublesome characteristic of measures of precision: whereas precision is commonly measured in standard deviation units (i.e., standard errors), most experts agree that efforts to maximize precision should focus on the squared standard error. Power, however, is a function of the standard error, rather than the squared standard error, and minimizing SE^2 is not equivalent to minimizing its square root. Although the mean standard error is lower for Option II than for Option I in all cases, the mean power associated with the set of contrasts is not. Power

is intimately related to the separation of the central and noncentral t distributions, i.e., the distance between their means along the baseline. The more distance there is between them, the easier it is to detect that difference, i.e., the greater the power. In the present context, a comparison of the baseline distances between the two distributions--quantified in the noncentrality parameter--reveals that the means of the noncentrality parameters are smaller for Option II than for Option I in the case of Helmert matrices. This indicates that the average distance between the two distributions is smaller for Option II; consequencly, it is more difficult to detect differences and the power is lower. This immediately suggests a need to consider developing an algorithm comparable to Option II, which minimizes the standard error, rather than the squared standard error. The overwhelming consensus to do the opposite should be carefully considered, however, before such an effort is undertaken.

Should an investigator choose to use one of the alternate allocation schemes, close attention should be paid to the constraints, particularly the homogeneity of variance assumption. The analysis of variance model does not provide for nonadditive treatment effects (Glass, et

al., 1972, pp. 240-241; Kirk, 1968, p. 52; Lindquist, 1953, pp. 75-58). Instead, they are hypothesized to be constant across individuals within groups, so the assumption of homoscedasticity is basically an assumption that the experimental material all comes from the same population. Since the homogeneity of variance assumption is central to both of these algorithms, anyone who implements them should decrease his/her chances of violating this fundamental assumption by randomly selecting sample material from a population and randomly assigning it to treatment conditions.

Though most of the advantages associated with Option II, in terms of relative efficiency, are similar, one notable exception occurs: a nonorthogonal pair of comparisons exhibits a relative efficiency of .75 (see Tables 9 and 10). This seems to indicate that Option II offers substantial advantages in some experiments, particularly when the number of planned comparisons is limited. It also points out that the investigator who focuses his/her attention on fewer hypotheses may reap significant power rewards.

The criterion of forcing contrasts to be tested at the same level of power (Option III), though certainly not a popular concept, has a great deal of intuitive appeal. Whether the indifference to such a criterion stems from a conscious decision to reject it or from a lack of awareness

that multiple contrasts may be estimated with widely disparate precision, is unknown. Perhaps the failure on the part of most researchers in education and psychology to explore power issues before selecting a research design is indicative that the latter is probably the case. Regardless of etiology, it would seem that this indifference might be examined (or re-examined, as the case may be).

Thematic throughout the findings of this investigation is the need for more definitive "pure" research before the techniques developed are routinely applied in field research. A broad investigation of a variety of coefficient matrices which would establish the general characteristics of studies best suited for unequal allocation (i.e., which offer the greatest power advantage over equal allocation) seems to be warranted. This process, coupled with a clear statement of the accompanying assumptions should permit reasonable guidelines to be established for the applied researcher who wishes to determine when unequal allocation will yield significant statistical benefits. Transcending these matters, however, is the considerable need for researchers in education and psychology to become aware of the fundamental procedures of power analysis, the a priori options implied by a consideration of these procedures, and, in particular, the implications of the failure to do so.

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APPENDIX A

ALLOCATING SAMPLE MATERIAL TO INCREASE PRECISION: MINIMIZING THE AVERAGE SE_{ψ}^2

The matrix in Table 1 (p. 34) represents an investigation consisting of k treatment groups ($j = 1, 2, \ldots, k$) and in which m a priori contrasts ($i = 1, 2, \ldots, m$) have been defined. Let N represent a fixed amount of sample material to be allocated among the k treatment groups, and n_j be the amount allocated to the jth group, i.e.,

$$\sum^{k} n_j = N \, . \tag{2-1}$$

By definition,

$$\sum^{k} c_{ij} = 0 \quad \text{for all i,} \tag{2-2}$$

and, for the purposes of this study,

$$\sum^{k} |c_{ij}| = 2 \quad \text{for all i.} \tag{2-3}$$

The standard error of a contrast (SE_{ψ_i}) is as follows:

$$SE_{\psi_i} = \sqrt{\sigma_e^2 \sum^{k} (c_{ij}^2 / n_j)} \, , \tag{2-4}$$

where σ_e^2 is the population within-group variance. Under the constraints of the present study, it is evident that the standard error of a contrast becomes a function only of the coefficients of the contrast and the amount of sample material in each group.

In order to maximize the precision for a <u>set</u> of contrasts, it has been deemed appropriate to minimize the average error variance, i.e., $SE_{\psi_i}^2$ (Ash & Hedayat, 1978, p. 1296; Bock, 1975, p. 307; Box & Wilson, 1951, p. 40; Feldt, 1958, p. 340; Finney, 1960, p. 11; Plackett & Burman, 1946, p. 307; Wald, 1943, p. 134):

$$(1 / m)\sum^{m}\sum^{k} (c_{ij}^2 / n_j) . \qquad (2\text{-}5)$$

By eliminating (1 / m), since it is a constant, and summing the columns of the matrix first, 2-5 may be rewritten

$$\sum^{k} (\sum^{m} c_{ij}^2 / n_j) . \qquad (2\text{-}6)$$

The goal, then, is to minimize the function F, where

$$F(n_1, n_2, \ldots, n_k) = \sum^{k} (\sum^{m} c_{ij}^2 / n_j) . \qquad (2\text{-}7)$$

Letting

$$d_j = \sum^{m} c_{ij}^2 , \qquad (2\text{-}8)$$

we want to minimize

$$F(n_1, n_2, \ldots, n_k) = \sum^{k} (d_j / n_j) , \qquad (2\text{-}9)$$

subject to the constraint

$$n_1 + n_2 + \ldots + n_k = N , \qquad (2\text{-}10)$$

which is equivalent to

$$(n_1 / N) + (n_2 / N) + \ldots + (n_k / N) = 1 . \qquad (2\text{-}11)$$

Let

$$x_j = (n_j / N) , \text{ and} \qquad (2\text{-}12)$$

$$n_j = N x_j . \qquad (2\text{-}13)$$

Substituting 2-13 into 2-9 and eliminating the constant (N), we now want to minimize the function F,

$$F(x_1, x_2, \ldots, x_k) = \sum^{k} (d_j / x_j) , \qquad (2\text{-}14)$$

subject to the constraint

$$x_1 + x_2 + \ldots + x_k = 1 . \qquad (2\text{-}15)$$

Using a LaGrange multiplier (Chow, 1962; Umland & Smith, 1959), we form the function G:

$$G(x_1, x_2, \ldots, x_k) = \sum^{k} (d_j / x_j) + \lambda (x_1 + x_2 + \ldots + x_k) . \qquad (2\text{-}16)$$

Taking the derivative of G with respect to each x_j and setting the results to zero,

$$\begin{aligned} \delta G / \delta x_1 &= (-d_1 / x_1^2) + \lambda = 0 \\ \delta G / \delta x_2 &= (-d_2 / x_2^2) + \lambda = 0 \\ &\vdots \\ \delta G / \delta x_k &= (-d_k / x_k^2) + \lambda = 0 . \end{aligned} \qquad (2\text{-}17)$$

Therefore,

$$\lambda = (d_1 / x_1^2) = (d_2 / x_2^2) = \ldots = (d_k / x_k^2) . \qquad (2\text{-}18)$$

Finding the reciprocals of each term and taking the square root,

$$y = (x_1 / d_1^{\frac{1}{2}}) = (x_2 / d_2^{\frac{1}{2}}) = \ldots = (x_k / d_k^{\frac{1}{2}}) , \qquad (2\text{-}19)$$

where

$$y = (1 / \lambda^{\frac{1}{2}}) . \qquad (2\text{-}20)$$

This means that

$$y = (x_1 / d_1^{\frac{1}{2}}) \quad \text{and} \quad x_1 = y d_1^{\frac{1}{2}}$$
$$y = (x_2 / d_2^{\frac{1}{2}}) \quad \text{and} \quad x_2 = y d_2^{\frac{1}{2}}$$
$$\vdots \qquad (2\text{-}21)$$
$$y = (x_k / d_k^{\frac{1}{2}}) \quad \text{and} \quad x_k = y d_k^{\frac{1}{2}} .$$

Since

$$x_1 + x_2 + \ldots + x_k = 1 \text{ , by 2-15 ,}$$
$$y d_1^{\frac{1}{2}} + y d_2^{\frac{1}{2}} + \ldots + y d_k^{\frac{1}{2}} = 1 , \qquad (2\text{-}22)$$

or

$$y(d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}}) = 1 , \qquad (2\text{-}23)$$

and

$$y = 1 / (d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}}) . \qquad (2\text{-}24)$$

Using 2-21 and substituting,

$$x_1 = d_1^{\frac{1}{2}} / (d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}})$$
$$x_2 = d_2^{\frac{1}{2}} / (d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}})$$
$$\vdots \qquad (2\text{-}25)$$
$$x_k = d_k^{\frac{1}{2}} / (d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}}) ,$$

or, in general,

$$x_j = d_j^{\frac{1}{2}} / (d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}}) . \qquad (2\text{-}26)$$

By using 2-12, substituting in 2-26, and multiplying by N,

$$n_j = (N d_j^{\frac{1}{2}}) / (d_1^{\frac{1}{2}} + d_2^{\frac{1}{2}} + \ldots + d_k^{\frac{1}{2}}) . \qquad (2\text{-}27)$$

Now, by substituting 2-8,

$$n_j = N \left(\sum^{m} c_{ij}^2\right)^{\frac{1}{2}} \div \left[\left(\sum^{m} c_{i1}^2\right)^{\frac{1}{2}} + \left(\sum^{m} c_{i2}^2\right)^{\frac{1}{2}} + \ldots + \left(\sum^{m} c_{ik}^2\right)^{\frac{1}{2}}\right] , \quad (2\text{-}28)$$

or,

$$n_j = \frac{N\sqrt{\sum^{m} c_{ij}^2}}{\sum^{k}\sqrt{\sum^{m} c_{ij}^2}} . \quad (2\text{-}29)$$

Once an investigator has constructed a matrix of contrast coefficients, then, he/she may use equation 2-29 to determine how much sampling material must be allotted to each treatment group, n_j, in order to minimize the average squared standard errors for the set of contrasts represented in the matrix. By doing so, the precision of the aggregate investigation is maximized, according to the criterion employed.

APPENDIX B

ALLOCATING SAMPLE MATERIAL TO DECREASE VARIABILITY: EQUALIZING THE SE_{ψ}^2

Using the generalized matrix of contrast coefficients contained in Table 1 (p. 34), along with the relationships described in Chapter II (represented by equations 2-1, 2-2, 2-3, and 2-4), a reasonable approach to equalizing the squared standard errors of all contrasts is an algebraic one. Under the constraints of this study, such equality can be achieved by setting $SE_{\psi_1}^2 = SE_{\psi_i}^2$, i.e.,

$$\sum^{k} (c_{1j}^2 / n_j) = \sum^{k} (c_{ij}^2 / n_j) , \qquad (3\text{-}1)$$

for all i (i = 2, 3, . . . , m), subject to the constraint, as in 2-15,

$$x_1 + x_2 + \ldots + x_k = 1 , \qquad (3\text{-}2)$$

where

$$x_j = n_j / N . \qquad (3\text{-}3)$$

It is apparent in such an approach that matrices of contrast coefficients which are not square (i.e., in which the number of contrasts is greater than or less than the number of treatment groups) may not yield usable solutions. If there are more rows than columns, the system is overspecified, and solutions can be derived only by happenstance. On the other hand, if there are more treatment groups than contrasts, solutions may not be unique, i.e., there could be infinite sets of solutions which satisfy

the equations. It would seem, therefore, that a modification of the approach should be used.

Accordingly, several varieties of matrices were tested in order to determine the characteristics of a usable matrix. It was discovered that nonorthogonal matrices yield usable unique or nonunique solutions only by chance, and that orthogonal matrices provide meaningful solutions--even nonunique--only in some cases. An example of an orthogonal matrix which yields no usable solutions is as follows:

$$\begin{bmatrix} 0 & 0 & 1 & -1 & 0 \\ 1 & 0 & 0 & 0 & -1 \\ 1/2 & 0 & -1/2 & -1/2 & 1/2 \\ 1/4 & -1 & 1/4 & 1/4 & 1/4 \end{bmatrix} \qquad (3\text{-}4)$$

Though there is an infinite number of solutions with this matrix, all lead to zero and/or negative allocations to one or more groups.

Only those matrices of what shall be deemed the "Helmert" type seem to yield viable solutions consistently; for example, using five treatment groups again,

$$\begin{bmatrix} 1 & -1/4 & -1/4 & -1/4 & -1/4 \\ 0 & 1 & -1/3 & -1/3 & -1/3 \\ 0 & 0 & 1 & -1/2 & -1/2 \\ 0 & 0 & 0 & 1 & -1 \end{bmatrix} \qquad (3\text{-}5)$$

This type of matrix seems to be common in the literature (Games, 1975b; Hays, 1981; Myers, 1979). Since the number

of columns--treatments--exceeds the number of rows--contrasts--the algebraic approach cited above provides nonunique solutions. It seems reasonable, therefore, to add an additional constraint, i.e., that the SE_{ψ}^2 not only be equal across contrasts, but that they also be at a minimum. The problem can be restated: we want to minimize

$$\sum^{k} (c_{1j}^2 / x_j) , \tag{3-6}$$

subject to 2-2, 2-3, 3-1, and 3-2.

By the method of LaGrange multipliers, we can define function F:

$$\begin{aligned} F(x_1, x_2, \ldots, x_k) = & \sum^{k} (c_{1j}^2 / x_j) \\ & + \lambda_1 (x_1 + x_2 + \ldots + x_k) \\ & + \lambda_2 \sum^{k} (c_{1j}^2 - c_{2j}^2) (1 / x_j) \\ & + \lambda_3 \sum^{k} (c_{1j}^2 - c_{3j}^2) (1 / x_j) + \ldots \\ & + \lambda_m \sum^{k} (c_{1j}^2 - c_{mj}^2) (1 / x_j) . \end{aligned} \tag{3-7}$$

A complete solution would require that F be differentiated with respect to each x_j and each resulting equation be set equal to zero. For the present purposes, however, we need to consider only the last two variables, x_{k-1} and x_k, since the patterns of solutions among the nonunique solution sets seemed to indicate that those solutions which allocated material equally to these two groups tend to produce SE_{ψ}^2 which are smaller than other alternatives.

$$\delta F / \delta x_{k-1} = (-c_{1,k-1}^2) / x_{k-1}^2 + \lambda_1 \qquad (3\text{-}8)$$
$$+ \lambda_2 (c_{1,k-1}^2 - c_{2,k-1}^2) (-1 / x_{k-1}^2)$$
$$+ \lambda_3 (c_{1,k-1}^2 - c_{3,k-1}^2) (-1 / x_{k-1}^2) + \ldots$$
$$+ \lambda_m (c_{1,k-1}^2 - c_{m,k-1}^2) (-1 / x_{k-1}^2) = 0 .$$

Likewise,

$$\delta F / \delta x_k = (-c_{1k}^2) / x_k^2 + \lambda_1 \qquad (3\text{-}9)$$
$$+ \lambda_2 (c_{1k}^2 - c_{2k}^2) (-1 / x_k^2)$$
$$+ \lambda_3 (c_{1k}^2 - c_{2k}^2) (-1 / x_k^2) + \ldots$$
$$+ \lambda_m (c_{1k}^2 - c_{2k}^2) (-1 / x_k^2) = 0 .$$

Multiplying 3-9 by $(-x_k^2)$,

$$c_{1k}^2 - \lambda_1 x_k^2 + \lambda_2 (c_{1k}^2 - c_{2k}^2) + \lambda_3 (c_{1k}^2 - c_{3k}^2)$$
$$+ \ldots + \lambda_m (c_{1k}^2 - c_{mk}^2) = 0 . \qquad (3\text{-}10)$$

In all matrices of the form 3-5, however,

$$|c_{j,k-1}| = |c_{jk}| , \qquad (3\text{-}11)$$

so

$$c_{j,k-1}^2 = c_{jk}^2 . \qquad (3\text{-}12)$$

Multiplying 3-8 by $(-x_{k-1}^2)$ and substituting 3-12,

$$c_{1k}^2 - \lambda_1 x_{k-1}^2 + \lambda_2 (c_{1k}^2 - c_{2k}^2) + \lambda_3 (c_{1k}^2 - c_{3k}^2)$$
$$+ \ldots + \lambda_m (c_{1k}^2 - c_{mk}^2) = 0 . \qquad (3\text{-}13)$$

Subtracting 3-13 from 3-10,

$$-\lambda_1 x_k^2 + \lambda_1 x_{k-1}^2 = 0 . \qquad (3\text{-}14)$$

Therefore,

$$x_{k-1}^2 = x_k^2 \quad \text{and} \quad x_{k-1} = x_k . \qquad (3\text{-}15)$$

Equation 3-15 also implies that, in order to meet the

criteria imposed on the problem, there must be equal sample material in groups k and k-1. This relationship provides the additional equation needed to determine unique solutions for Helmert matrices, in that it represents the kth equation for a problem which has k unknowns.

Because the pattern of Helmert matrices is regular regardless of the number of treatment groups, it is possible to devise a simple means for determining optimal allocation to achieve equal precision among contrasts. A FORTRAN computer program written by Rhyne (1970) was used to solve the systems of linear equations for designs consisting of up to 20 treatment groups. Table 2 provides values necessary to determine optimal allocation using the present criteria by applying the following formula:

$$n_j = \left[r_q \Big/ \sum_{}^{k} r \right] N \quad , \tag{3-16}$$

where

$$q = k - j + 1 \quad , \tag{3-17}$$

and r represents the proportion of sample material to be included in group q for each unit included in group k. The use of 3-16 under the conditions cited ensures that each Helmert contrast is tested with equal precision, and that this precision is at a maximum.

APPENDIX C

Table 1

A General Matrix of Coefficients for Planned Comparisons[a]

Contrast	Treatment Group 1	2	. . .	j	. . .	k
ψ_1	c_{11}	c_{12}	. . .	c_{1j}	. . .	c_{1k}
ψ_2	c_{21}	c_{22}	. . .	c_{2j}	. . .	c_{2k}
.	.	.		.		.
.	.	.		.		.
.	.	.		.		.
ψ_i	c_{i1}	c_{i2}	. . .	c_{ij}	. . .	c_{ik}
.	.	.		.		.
.	.	.		.		.
.	.	.		.		.
ψ_m	c_{m1}	c_{m2}	. . .	c_{mj}	. . .	c_{mk}

[a]Of the general form,
$c_{i1}\mu_1 + c_{i2}\mu_2 + \ldots + c_{ij}\mu_j + \ldots + c_{ik}\mu_k$.

Table 2

Allocating Sample Material[a] to Achieve Equal Precision in a Matrix of Helmert Contrasts[b]

r^c	Group[d] q	$\sum_{k} r$	r^c	Group[d] q	$\sum_{k} r$
1.0000	1	1.0000	.5426	11	7.2283
1.0000	2	2.0000	.5391	12	7.7674
.6667	3	2.6667	.5361	13	8.3035
.6207	4	3.2874	.5336	14	8.8371
.5950	5	3.8824	.5314	15	9.3685
.5786	6	4.4610	.5295	16	9.8980
.5671	7	5.0281	.5186	17	10.4166
.5586	8	5.5867	.5172	18	10.9338
.5521	9	6.1388	.5159	19	11.4497
.5469	10	6.6857	.5147	20	11.9644

[a] Using the formula $n_j = N\ (r_q \ / \ \sum_{k} r)$.

[b] Arranged so that $\sum_{m}|c_{i1}| \leq \sum_{m}|c_{i2}| \leq \cdots \leq \sum_{m}|c_{ik}|$.

[c] Proportion of sample material in q for each unit in k.

[d] $q = k - j + 1$.

Table 3

Precision Comparisons Between Two Allocation Schemes
Nonorthogonal, k = 3, N = 30

	Allocation Criterion			
	Equal n_j [b]		Min.Avg.SE^2 [c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.4472	.1450	.4647	.1361
2	.3873	.1860	.3806	.1919
3	.4472	.1450	.4264	.1572
Mean	.4272	.1587	.4239	.1617
Sigma	.0282	.0193	.0344	.0230

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1 & 0 \\ 1/2 & 1/2 & -1 \\ 1 & 0 & -1 \end{bmatrix}$$

[b] n_1=10; n_2=10; n_3=10.

[c] n_1=11; n_2=8; n_3=11.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 4

Precision Comparisons Between Two Allocation Schemes
Nonorthogonal, k = 3, N = 45

	Allocation Criterion			
	Equal n_j [b]		Min.Avg.SE^2 [c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.3652	.2129	.3770	.2009
2	.3162	.2774	.3131	.2826
3	.3652	.2129	.3483	.2321
Mean	.3489	.2344	.3461	.2385
Sigma	.0231	.0304	.0261	.0337

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1 & 0 \\ 1/2 & 1/2 & -1 \\ 1 & 0 & -1 \end{bmatrix}$$

[b] n_1=15; n_2=15; n_3=15.

[c] n_1=17; n_2=12; n_3=16.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 5

Precision Comparisons Among Three Allocation Schemes
Helmert, k = 4, N = 40

	Allocation Criterion					
	Equal n_j[b]		Min.Avg.SE^2[c]		"Equal" SE^2[d]	
Contrast[a]	SE[e]	Power[f]	SE[e]	Power[f]	SE[e]	Power[f]
1	.3651	.2111	.3790	.1973	.3967	.1819
2	.3873	.1898	.3957	.1827	.4082	.1729
3	.4472	.1478	.4264	.1603	.4082	.1729
Mean	.3999	.1829	.4004	.1801	.4044	.1759
Sigma	.0347	.0263	.0196	.0152	.0054	.0042

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/3 & -1/3 & -1/3 \\ 0 & 1 & -1/2 & -1/2 \\ 0 & 0 & 1 & -1 \end{bmatrix}$$

[b] $n_1=10$; $n_2=10$; $n_3=10$; $n_4=10$.

[c] $n_1=9$; $n_2=9$; $n_3=11$; $n_4=11$.

[d] $n_1=8$; $n_2=8$; $n_3=12$; $n_4=12$.

[e] σ_e^2 is assumed to be 1.

[f] For medium effect size; global alpha is 0.05.

Table 6

Precision Comparisons Among Three Allocation Schemes
Helmert, k = 4, N = 60

	Allocation Criterion					
	Equal n_j[b]		Min.Avg.SE^2[c]		"Equal" SE^2[d]	
Contrast[a]	SE[e]	Power[f]	SE[e]	Power[f]	SE[e]	Power[f]
1	.2981	.3137	.3053	.3000	.3239	.2689
2	.3162	.2812	.3204	.2744	.3333	.2550
3	.3651	.2157	.3536	.2288	.3333	.2550
Mean	.3265	.2702	.3264	.2677	.3302	.2596
Sigma	.0283	.0408	.0202	.0294	.0044	.0066

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/3 & -1/3 & -1/3 \\ 0 & 1 & -1/2 & -1/2 \\ 0 & 0 & 1 & -1 \end{bmatrix}$$

[b] $n_1=15$; $n_2=15$; $n_3=15$; $n_4=15$.

[c] $n_1=14$; $n_2=14$; $n_3=16$; $n_4=16$.

[d] $n_1=12$; $n_2=12$; $n_3=18$; $n_4=18$.

[e] σ_e^2 is assumed to be 1.

[f] For medium effect size; global alpha is 0.05.

Table 7

Precision Comparisons Between Two Allocation Schemes
Orthogonal, k = 4, N = 40

	Allocation Criterion			
	Equal n_j [b]		Min.Avg.SE^2 [c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.3651	.2111	.3376	.2437
2	.3162	.2750	.3203	.2686
3	.3162	.2750	.3203	.2686
Mean	.3325	.2537	.3261	.2603
Sigma	.0231	.0301	.0082	.0117

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/3 & -1/3 & -1/3 \\ 1/2 & -1/2 & -1/2 & 1/2 \\ 1/2 & 1/2 & -1/2 & -1/2 \end{bmatrix}$$

[b] n_1=10; n_2=10; n_3=10; n_4=10.

[c] n_1=13; n_2=9; n_3=9; n_4=9.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 8

Precision Comparisons Between Two Allocation Schemes
Orthogonal, k = 4, N = 60

	Allocation Criterion			
	Equal n_j [b]		Min.Avg.SE^2 [c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.2981	.3137	.2707	.3750
2	.2582	.4085	.2638	.3929
3	.2582	.4085	.2638	.3929
Mean	.2715	.3769	.2661	.3869
Sigma	.0188	.0447	.0033	.0084

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/3 & -1/3 & -1/3 \\ 1/2 & -1/2 & -1/2 & 1/2 \\ 1/2 & 1/2 & -1/2 & -1/2 \end{bmatrix}$$

[b] $n_1=15$; $n_2=15$; $n_3=15$; $n_4=15$.

[c] $n_1=21$; $n_2=13$; $n_3=13$; $n_4=13$.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 9

Precision Comparisons Between Two Allocation Schemes
Nonorthogonal, k = 4, N = 40

	Allocation Criterion			
	Equal n_j [b]		Min.Avg.SE^2 [c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.3651	.2650	.3450	.2910
2	.4472	.1915	.3563	.2758
Mean	.4062	.2283	.3507	.2834
Sigma	.0411	.0368	.0057	.0076

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/3 & -1/3 & -1/3 \\ 1 & 0 & 0 & -1 \end{bmatrix}$$

[b] $n_1=10$; $n_2=10$; $n_3=10$; $n_4=10$.

[c] $n_1=18$; $n_2=4$; $n_3=4$; $n_4=14$.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 10

Precision Comparisons Between Two Allocation Schemes
Nonorthogonal, k = 4, N = 60

	Allocation Criterion			
	Equal n_j [b]		Min.Avg.SE^2 [c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.2981	.3775	.2817	.4145
2	.3651	.2694	.2910	.3930
Mean	.3316	.3235	.2864	.4038
Sigma	.0335	.0541	.0047	.0108

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/3 & -1/3 & -1/3 \\ 1 & 0 & 0 & -1 \end{bmatrix}$$

[b] n_1=15; n_2=15; n_3=15; n_4=15.

[c] n_1=27; n_2=6; n_3=6; n_4=21.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 11

Precision Comparisons Among Three Allocation Schemes
Helmert, k = 6, N = 60

	Allocation Criterion					
	Equal n_j[b]		Min.Avg.SE^2[c]		"Equal" SE^2[d]	
Contrast[a]	SE[e]	Power[f]	SE[e]	Power[f]	SE[e]	Power[f]
1	.3464	.1789	.3617	.1638	.3808	.1476
2	.3535	.1716	.3674	.1587	.3854	.1442
3	.3651	.1607	.3615	.1640	.3749	.1523
4	.3873	.1428	.3814	.1472	.3867	.1432
5	.4472	.1081	.4264	.1183	.3922	.1392
Mean	.3799	.1524	.3797	.1504	.3840	.1453
Sigma	.0364	.0253	.0245	.0172	.0058	.0044

[a] For the coefficient matrix

$$\begin{bmatrix} 1 & -1/5 & -1/5 & -1/5 & -1/5 & -1/5 \\ 0 & 1 & -1/4 & -1/4 & -1/4 & -1/4 \\ 0 & 0 & 1 & -1/3 & -1/3 & -1/3 \\ 0 & 0 & 0 & 1 & -1/2 & -1/2 \\ 0 & 0 & 0 & 0 & 1 & -1 \end{bmatrix}$$

[b] n_1=10; n_2=10; n_3=10; n_4=10; n_5=10; n_6=10.

[c] n_1=9; n_2=9; n_3=10; n_4=10; n_5=11; n_6=11.

[d] n_1=8; n_2=8; n_3=9; n_4=9; n_5=13; n_6=13.

[e] σ_e^2 is assumed to be 1.

[f] For medium effect size; global alpha is 0.05.

Table 12

Precision Comparisons Among Three Allocation Schemes
Helmert, k = 6, N = 90

	Allocation Criterion					
	Equal n_j[b]		Min.Avg.SE^2[c]		"Equal" SE^2[d]	
Contrast[a]	SE[e]	Power[f]	SE[e]	Power[f]	SE[e]	Power[f]
1	.2828	.2786	.3000	.2461	.3112	.2280
2	.2886	.2669	.2957	.2538	.3150	.2222
3	.2981	.2494	.3025	.2419	.3108	.2286
4	.3162	.2204	.3100	.2298	.3193	.2160
5	.3651	.1638	.3430	.1861	.3162	.2204
Mean	.3102	.2358	.3102	.2315	.3145	.2230
Sigma	.0297	.0410	.0170	.0240	.0032	.0047

[a] For the Coefficient matrix

$$\begin{bmatrix} 1 & -1/5 & -1/5 & -1/5 & -1/5 & -1/5 \\ 0 & 1 & -1/4 & -1/4 & -1/4 & -1/4 \\ 0 & 0 & 1 & -1/3 & -1/3 & -1/3 \\ 0 & 0 & 0 & 1 & -1/2 & -1/2 \\ 0 & 0 & 0 & 0 & 1 & -1 \end{bmatrix}$$

[b] n_1=15; n_2=15; n_3=15; n_4=15; n_5=15; n_6=15.

[c] n_1=13; n_2=14; n_3=14; n_4=15; n_5=17; n_6=17.

[d] n_1=12; n_2=12; n_3=13; n_4=13; n_5=20; n_6=20.

[e] σ_e^2 is assumed to be 1.

[f] For medium effect size; global alpha is 0.05.

Table 13

Precision Comparisons Between Two Allocation Schemes
Orthogonal, k = 6, N = 60

	Allocation Criterion			
	Equal n_j[b]		Min.Avg.SE^2[c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.2579	.3637	.2726	.3261
2	.3873	.1634	.3597	.1883
3	.4472	.1252	.3922	.1595
4	.3873	.1634	.4174	.1419
Mean	.3699	.2039	.3605	.2040
Sigma	.0691	.0936	.0547	.0724

[a] For the coefficient matrix

$$\begin{bmatrix} 1/3 & 1/3 & 1/3 & -1/3 & -1/3 & -1/3 \\ 1/2 & 1/2 & -1 & 0 & 0 & 0 \\ 1 & -1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 & -1/2 & -1/2 \end{bmatrix}$$

[b] n_1=10; n_2=10; n_3=10; n_4=10; n_5=10; n_6=10.

[c] n_1=13; n_2=13; n_3=11; n_4=11; n_5=6; n_6=6.

[e] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.

Table 14

Precision Comparisons Between Two Allocation Schemes
Orthogonal, k = 6, N = 90

	Allocation Criterion			
	Equal n_j[b]		Min.Avg.SE^2[c]	
Contrast[a]	SE[d]	Power[e]	SE[d]	Power[e]
1	.2106	.5386	.2182	.5060
2	.3162	.2470	.2943	.2850
3	.3651	.1860	.3333	.2224
4	.3162	.2470	.3299	.2271
Mean	.3020	.3047	.2939	.3101
Sigma	.0564	.1373	.0463	.1157

[a] For the coefficient matrix

$$\begin{bmatrix} 1/3 & 1/3 & 1/3 & -1/3 & -1/3 & -1/3 \\ 1/2 & 1/2 & -1 & 0 & 0 & 0 \\ 1 & -1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 & -1/2 & -1/2 \end{bmatrix}$$

[b] n_1=15; n_2=15; n_3=15; n_4=15; n_5=15; n_6=15.

[c] n_1=18; n_2=18; n_3=17; n_4=17; n_5=10; n_6=10.

[d] σ_e^2 is assumed to be 1.

[e] For medium effect size; global alpha is 0.05.